Peer Review Report for

"Development and Testing of Recreational Fishing Effort Surveys, Testing a Single-Phase Mail Survey Design"

Reviewed by

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Introduction

This document combines the comments provided by three different peer reviewers of the MRIP Project Report entitled "*Development and Testing of Recreational Fishing Effort Surveys, Testing a Single-Phase Mail Survey Design*." The document provides verbatim reviewer comments without identifying the source of each comment.

Reviewer 1

This review of the report entitled "Development and Testing of Recreational Fishing Effort Surveys: Testing a Single-Phase Mail Survey Design" provides comments and suggestions on the methods, results and conclusions found in the report. The review does not include any working with the original data and thus does not encompass any validation of data or primary calculations with the data. The review examines only summary calculations found in the report and, accepting those as shown, assesses the reasonableness of methods, approach and use of results to reach conclusions about aspects of Recreational Fishing Effort Surveys (RFES), especially the recommendation to move to a mail survey design.

The report presents the results of an evaluation of a single phase mail survey design as an alternative to the Coastal Household Telephone Survey (CHTS) for estimating marine recreational fishing effort. The objectives identified in the report were to:

1) test the feasibility of a mail survey design for collecting recreational fishing effort data and estimating fishing effort for shore and private boat anglers,

2) compare single phase mail survey and CHTS results, including metrics of survey quality and estimates of marine recreational fishing activity,

3) describe, to the greatest extent possible, differences between single phase mail survey and CHTS estimates in terms of sources of survey error, and

4) provide recommendations for follow-up action, including implementation of improved survey methods.

This review will discuss the objectives in order and provide several other insights to conclude.

Generally, the analysis is done very well with considerable thought about identifying and measuring sources of differences between the surveys. I find no meaningful issues in the methodology used or the analyses and therefore provide brief comments on the 4 objectives above and I do not reiterate the various findings. Finally, I will discuss some ideas for future consideration.

OBJECTIVE 1) test the feasibility of a mail survey design for collecting recreational fishing effort data and estimating fishing effort for shore and private boat anglers

The authors (Andrews, Brick and Mathiowetz) describe a well conceived experimental approach to providing metrics to lead to decisions on survey approaches. They describe problems with the existing survey, especially low response rates, and identify issues that can further degrade quality of the existing design e.g., declining landline use. They make reasoned and convincing arguments, supported by the metrics, that response rates and response error are less of a problem with mail surveys and those improvements also reduce bias problems. The authors also show that the quality improvements can be achieved within the time frame required of the survey operations. I agree with their conclusion that a mail survey design is feasible and preferred.

The use of a $2 incentive was clearly justified by the analysis of experiments found in appendix B. Often incentive experiments fail to discuss overall cost relative to effect. Here, the authors provide a fair comparison taking cost into consideration. Further analysis of the impact on broader survey costs including the typically expensive follow up of nonrespondents for incremental incentives from $2 to $5 would add to the understand, but the gains in response at the $2 level would typically be cost effective, making the use in the design reasonable.

OBJECTIVE 2) compare single phase mail survey and CHTS results, including metrics of survey quality and estimates of marine recreational fishing activity

The research appropriately examines design features that may impact differences between survey approaches. The analysis indicates that mail survey methods result in larger estimates of percent of households fishing while mean numbers of within household statistics vary with mean trips larger for mail and other items not particularly different. Reasons for the differences are hypothesized and explored in a balanced and fair manner.

While "quality" is not specifically defined in the report, most methodologists would consider cost, timeliness and relevance along with the usual focus on error sources. The authors have exhibited some cost improvements in the mail survey approach and that it meets timeliness needs. The authors explore various thoughts on response differences and bias sources (geographic, unlicensed anglers, etc.) finding that the mailing methods perform well and the responses may be more in line with the concepts desired.

OBJECTIVE 3) describe, to the greatest extent possible, differences between single phase mail survey and CHTS estimates in terms of sources of survey error

As mentioned above, survey error is one of the quality dimensions. The report explores usual sources of error for the survey types. Identifying sources of error is an intuitive and experience based endeavor. The authors were creative and explored a commendable range of ideas. The range of finding are sufficient to support their conclusions regarding survey methodology changes.

OBJECTIVE 4) provide recommendations for follow-up action, including implementation of improved survey methods

The matching of ABS sample to license frames (p. 8) is a good idea and can be effective for stratification and sample allocation.

The main recommendation, using a single-phase mail survey, covers many potential improvements. This recommendation is supported and reasonable. The suggestion for continued development and testing (p. 32) is reasonable because there usually are changes to consider when moving to full scale implementation.

With the evolution of e-mail and web collection modes, the recommendation to explore such methods is reasonable. Methodologists such as Don Dillman are conducting current research that should be examined for applicability.

COMMENTS

Bottom line, I can find nothing of concern in the methods, analyses or conclusions in the paper. That said, identifying error sources in surveys is difficult, but the authors explored a wide and thoughtful set of issues and make appropriate suggestions for further research. As such, I find no reason to be concerned about their suggestion to move to a mail survey approach and believe it would be a reasonable thing to do.

IDEAS

Consider development of a bridging survey approach. Estimates will be changing with a move to mail and the research is based on a subset of areas to be sampled. Methodology will likely evolve a bit as well. A bridge helps to keep the time series of estimates usable.

The may be a number of co varying attributes related to response and fishing. Age comes to mind as it is likely related to landline or cell use. It may also be something that increases with age to a point at which infirmity reduces fishing. The age distribution in the study states may be impacting some of the results. FL and NC are more destination states for retirees from the north. Thus, age may be influencing some of the state differences found (e.g. Table 4) and mail could reduce the impact in states with an older population.

The analysis of difference from APAIS should consider the non-coastal travelers reason to travel and method of travel. Someone driving can take poles for surf fishing and avoid piers etc. Those flying have a much more difficult time taking equipment. This could influence the APAIS results. Also some areas are more known for travel to surf fish - NC - and travel there may be more by personal vehicle and with gear. Other areas like Florida may be more by air travel.

I'm not sure that I agree with footnote 15. I've never had a problem finding a non-APAIS place to surf fish near the hotel or condo wherever we stay. It may be instructive to look at differences by state for domain estimates for in-state vs. out-of-state people in the APAIS data.

Another factor to consider may be the proportion of the state's population living near the coast. If large cities are coastal, surf fishing may dominate.

The thought in the above comments is that other characteristics may be useful in further improving the survey design and information useful to collect. Exploring how fishing responses compare to other characteristics collected in the survey may provide more ideas.

Pay pier is not specifically mentioned in the questionnaire in Q 15a or b. Dock etc of 15a may not draw the memory out. I might not have considered the fishing pier experience when answering 15a and then it is not a part of 15b.

Reviewer 2

"Developing and Testing of Recreational Fishing Effort Survey Testing a Single Phase Mail Survey Design" reports on research designed to improve the way estimates of recreational fishing effort are made with an emphasis on the last test conducted in four states using what the authors call a "single-phase dual-frame mail survey." The research itself is sturdy and the results (that the new estimation strategy is far superior to what is done now) convincing. The report itself, however, has a number of flaws.

One flaw that afflicts many research reports is the inconsistent use of tense. This is understandable given that the research has already been done but the methods used can be repeated, so describing them in the present tense makes some sense. What makes the tense-use problem particularly acute here is that some of the methods described were tested before the method on which the report focuses. The reader would have an easier time understanding what is old and what is new if the past perfect where used ("anglers *had been* mailed") in describing previous methods tested. Instead, the present is used to describe a method that had been tested before the single-phase dual-frame mail survey, while single-phase dual-frame mail survey is later described in the past tense.

A second flaw is that the authors' single-phase dual-frame mail survey, although a mail survey, is not single phase (there is subsampling in certain strata) and only technically dual frame. There *are* two frames in a state, an address-based resident frame and a frame containing non-resident licensed saltwater anglers, but since these frames do not overlap, dual-frame methodology is not employed. Instead, these separate frame as used in creating disjoint strata.

There is much discussion of stratification, but not enough to satisfy this reader. What exactly were the strata in each state, the targeted stratum sampling rates, and the actual stratum response rates? Readers are lead to believe that weights were equal within strata and reflected both the within-stratum sampling and response rates but are never told so explicitly. Consequently, that reasonable approach to handling nonresponse is never justified. (The lack of

details carries over to Appendix B, where readers are given very little information about a logistic regression used to draw many conclusions.)

There is one minor technical error (excusing the use of "single-phase" because there is only a single phase of data collection) and a somewhat larger technical embarrassment in the report. The minor technical error is the suggestion on page 25 that the expectation operator on probability-sampling theory breaks down for very small prevalences. It does not, estimates remain unbiased. The problem is that they are not very accurate. Their relative variances are high, and their nonnormality makes coverage-interval construction from their variance estimates dubious.

The somewhat larger embarrassment is that, contrary to the authors' assertion, the fraction of respondents engaged in fishing is not a reasonable measure of the efficiency of the single-phase-dual-frame-mail-survey estimation strategy because targeted anglers are down-weighted in the estimation. Good measures of the strategy's relative statistical efficiency are the design effects of the estimates it produces. The only design effect the authors report is, unfortunately, close to 1. Others, especially for estimates of the anglers themselves, are likely to be smaller (if correctly computed for the purpose of evaluating the design).

Ultimately, however, these criticisms of the report are minor. As I wrote earlier, I found the report's conclusions convincing. I very much like what I can make out of the sampling and estimation strategy that the authors' recommend. The flaws in the report are statistical in nature. On the survey-methodology side, the report contains a commendable treatment of the problems and limitations involved in collecting the information desired.

Reviewer 3

This well written and thoughtful report makes its main case overwhelmingly. The single phase mail survey (SPMS) is the clear winner when compared to the Coastal Household Telephone Survey (CHTS).

Given the stark differences in marine fishing activity reported by the two surveys, there will be keen interest in how the differences break out by age, racial/ethnic, and sex groups. Are the young and elderly fishing off piers sometimes being missed? Are women and girls sometimes regarded as participants in marine fishing and other times just thought of as on-lookers? Do we know that racial/ethnic minorities are being represented fairly? There doubtless will be great interest in such questions.

Specific Comments:

Page 12, lines 5-7 from bottom: "median" is not explained correctly. It means that half the responses were received before the 14^{th} day (or possibly on the 14^{th} day, depending on the specifics of the definition).

On page 13, Figure 1, I did not understand the dots. There are many more dots after 20 days than before.

The last paragraph on page 23 makes perfect sense right up to the final "i.e.". The phrase "i.e., only individuals in households without licensed anglers could have contributed to nonresponse bias resulting from differential response between anglers and non-anglers" does not seem to me to follow from the rest of the paragraph nor do I think it is true. On rereading this some time after I wrote the previous two sentences, the point may be that unlicensed anglers mess up the nonresponse adjustment. I still do not think the quoted sentence is the right way to say it.

I disagree with the argument at the end of the first complete paragraph on page 25: "…we hypothesized that low sample sizes in the CHTS during low-activity waves result[s] in underestimates of state-level fishing effort." Small sample sizes will increase variance but not cause bias. It could happen that one would get a larger than average number (e.g. 2) of anglers, and they would have large weights.

I kept wanting to see discussion of possible measurement bias, and finally there is an excellent discussion in the paragraph beginning on page 28. But measurement bias could affect the earlier analyses so should be introduced sooner.

It is remarkable (page B8, Table 6) that the $1 and $2 incentives lead to lower relative costs per completed survey compared to no incentive or $5 incentive. But I do not think one can conclude that the $5 incentive is sub-optimal (last line on page B8). It depends on the relative value one puts on maximizing response rates versus minimizing data collection costs. Even though (page B7, Table 5) the prevalence rate estimates do not differ significantly among the incentive levels, other estimates may be enhanced by a higher response rate.

Editorial Comments:

Executive Summary, line 4: Either delete semi-colon or replace with colon.

On page 18, line 3 of second paragraph: I would change "(wireless households)" to "(wireless only households)".

Page 25, last line of first complete paragraph: Change "results" to "result".

Page 33, second reference: I think the %20s in the URL should be spaces. Some systems changes spaces to %20s.

Page B5, Table 2, $2 Incentive line: Change "36" to "36.0".